on the comparison, especially if these serve to explain important discrepancies, clarify inaccuracies and advance understanding in general. If such an activity is pursued in good faith and with rigorous intellectual honesty the scientific community will be better served and public opinion concerning its research endeavors substantially enhanced.

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Authors' Reply

THE AIM of our paper [1] was to compare different existing low-Reynolds-number turbulence models, using the same numerical code, for the turbulent boundary layer along the heated vertical plate. One of the models compared was the low-Reynolds-number model of To and Humphrey. In their Letter to the Editor, Humphrey and To criticize some aspects of our paper.

We agree with their remark (4) that our Fig. 3 shows that we did not satisfy the restriction $y^+ > 11.5$ when applying the standard k–ε model. We actually applied the wall function for k and ε in the standard k–ε model at the first inner computational grid point, not necessarily with $y^+ > 11.5$. This is not very clear from the text; we only ended Section 2 with the remark that most natural convection calculations take the first inner grid point at $y^+ < 11.5$.

The main criticism of Humphrey and To on our paper is, however, their suggestion (remark (1)) that we incorrectly implemented their To and Humphrey low-Reynolds-number model, causing a serious error in our Table 2. Humphrey and To want to "alert the readers of Henkes and Hoogendoorn about a serious inconsistency in their work". We prefer to make clear that there seems to be an inconsistency *between* one of our calculations and a calculation of To and Humphrey [2]. We also referred to this inconsistency in our paper (Section 4): "Present results agree up to a graphical accuracy, except for the results with the To and Humphrey model, which considerably deviate".

In our study we extensively checked that the numerical results presented in our paper indeed seem to be accurate solutions of the equations listed:

(i) we thoroughly checked that the discretized equations were correctly coded;
(ii) we checked the accuracy of the numerical results by grid refinement;
(iii) we compared with solutions of other authors, using their models.

Of course, because of the criticism, we once again checked the correctness of the implementation of the To and Humphrey model in our code. We also recalculated the solution and refined the grid from 25×25 to 400×400 grid points (the distribution of grid points chosen is slightly different from ref. [1]). Table 1 gives Nu_x and the value of ε at the wall for $Gr_x = 10^{11}$ (and $Pr = 0.72$) using the low-Reynolds-number model of To and Humphrey. The grid-independent value of Nu_x is 674, which is only 0.7% below the value listed in Table 2 of ref. [1]. Further, as expected, ε_w remains finite at the wall. This clarifies remarks (2) and (3) of Humphrey and To: we did not incorrectly apply a wall function for ε_w, but evaluated the expression $\varepsilon_w = 2\nu(\partial k^{1/2}/\partial y)^2_w$, as dictated by the To and Humphrey model. The numerical value resulting for ε_w does not become unbounded, but is simply too large to be visible in our Fig. 3. It is strange that Humphrey and To are perplexed by our sharp increase of ε in the inner layer. Figure 10 of To and Humphrey [2] in which ε is plotted as part of the energy budget of the k-equation, shows the same behaviour: also here we see that ε for $y \to 0$ grows too fast to get ε_w in the figure.

As we described in Section 4 of ref. [1], our code was checked to recalculate the low-Reynolds-number results of Patel *et al.* for the forced-convection boundary layer. Also the results of Cebeci and Khattab and Lin and Churchill for the hot vertical plate were recalculated. Hence all these calculations checked with the results of other authors, whereas only the result as published by To and Humphrey [2] could not be reproduced by our code: at $Gr_x = 10^{11}$ they find $Nu_x = 550$, whereas we find a 23% larger value. Indeed the value calculated by To and Humphrey is very close to the experimental value. To and Humphrey checked the numerical accuracy of their result by only slightly refining the grid

Table 1. Code of Henkes and Hoogendoorn applying the To and Humphrey model ($Gr_x = 10^{11}$, $Pr = 0.72$)

Grid	Nu_x	$\dfrac{\varepsilon_w \nu}{(g\beta\Delta T\nu)^{4/3}}$
25×25	664.9	10.42
50×50	671.0	11.99
100×100	673.3	12.29
200×200	674.0	12.35
400×400	673.8	12.38

from 52×36 to 52×47 points (this grid refinement leads to a 0.4% change in the heat transfer). We suggest that further grid refinement, say up to 104×72 points, can help to convince that the result of To and Humphrey is sufficiently grid independent. We end up with only two reasons that might account for the difference between our results and those of To and Humphrey: (a) To and Humphrey solved the full elliptic problem, whereas we applied boundary-layer simplifications, and (b) To and Humphrey also included non-Boussinesq efffects, whereas we implemented the Boussinesq limit. As the Grashof number is large, it is not probable that the boundary-layer approach accounts for the difference. The Boussinesq approximation holds provided the overheat ratio $\Delta T/T_\infty$ is sufficiently small. The calculation presented by To and Humphrey corresponds to an overheat ratio of 0.1911. The best-fit experimental relation of Siebers *et al.* (also referred to as equation (1) in the paper of To and Humphrey) says that Nu_x is proportional to $Gr_x^{1/3} \times (1+\Delta T/T_\infty)^{-0.14}$. Therefore, using the Boussinesq approximation for an overheat ratio of 0.1911 overpredicts the wall-heat transfer by less than 3%, which is also insufficient to account for the difference between our calculation and the calculation of To and Humphrey.

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